and will, by means of the reproduction of photographs, illustrate some of the more important physiographic features of Central Australia.
Baldwin Spencer.

Palæontology at the Royal School of Mines.

In reading the excellent review of the biography of Sir R. Owen, which appeared in last week's Nature, I observe an error which, though small, requires correction. It is stated that when Owen surrendered his appointments at the College of Surgeons he was "enabled to accept the lectureship on Palæontology at the Royal School of Mines, in 1857." The records of that Institution will show that Owen never held a lectureship there, nor was he in any way connected with the School.

The large theatre of the Museum of Practical Geology was frequently employed for other purposes than those of the School, by permission of the Director-General; and it was in virtue of such permission from Sir Roderick Murchison that Prof. Owen used the theatre for the delivery of his lectures on Palæontology to the public, in 1857 and subsequently.
T. H. Huxley.

Eastbourne, December 27, 1894.

Eocene Fossils at Murren.

I have read with surprise the extracts from the letter which you have received from Dr. Fellenberg on this subject. Of course it has long been known that there are Eocene strata at Murren. But below them lie calcareous rocks coloured on the Swiss map as Malm. These are so described on p. 211 of the "Livret Guide," published by the International Geological Congress which met this autumn at Zürich. During the subsequent excursion, under the able guidance of Prof. Renevier, Prof. Golliez, and M. Lugeon, we were taken to Murren and shown these rocks, and Prof. Golliez gave us the reasons which had led some geologists to regard them as Trias rather than Malm. It was in these calcareous beds that the layer containing nummulites was met with. The train was just starting, and we had to leave, but the find excited so much interest that M. Lugeon returned to Murren the next day, with some of the party, and verified the exact locality.
John Lubbock.

High Elms, Farnborough.

The Use of the Globe in Crystallography.

In your issue of December 20, Mr. Buchanan revives a graphic method of crystallographic calculation which seems to have been used in the early part of the century (*vide* "Zur physischen Krystallonomie, &c.," Grassmann, 1829, p. 37), and claims that by use of the globe and *métrosphère* "every problem in the geometry of crystals can be solved with ease and accuracy."

Crystallographic angular measurements are said to be accurate if subject to a probable error of less than two or three minutes, and descriptions of inorganic substances are nowadays habitually published in which the probable errors are of this order. Although no details of Mr. Buchanan's method are given, it seems inconceivable that any graphic process of crystallographic calculation, involving triangulation on a sphere, could be accurately performed without the use of numberless tedious precautions and large and cumbrous apparatus.

It is usually more easy to grasp a good plane diagram of any solid figure than to understand and follow up explanations on the solid figure itself; the use of the latter is liable to lead to inaccuracy of expression and confusion of thought. Thus, when the sphere is used, the real meaning of the points which Mr. Buchanan describes as cataloguing the edges occurring on crystals, is not at once seen; using the plane projection, it is immediately apparent that these points are characteristic, not merely of the edges, but in a much wider sense, of the zones: they are merely the poles of the zone circles.

Further, the positions of the points representing the corners are dependent on the *sizes* of the faces concerned, which, as we teachers of crystallography are at infinite pains to impress on our students, have no crystallographic signification; these points, then, are not *characteristic* of the corners.

Similarly, the rather complicated piece of reasoning respecting "reciprocal inversion forms" simply yields the well-known result that in the cubic system, the octahedron truncates the corners of the cube.
William J. Pope.

Central Technical College, South Kensington.

"The Zoological Record."

It has long been a matter of regret that the *Zoological Record* is not sold in separate parts; a specialist requiring any one part being required to pay for all the others, though they may be of no more use to him than so much waste-paper.

In order to remedy, to some extent, this unfortunate state of things, I am proposing (if sufficient support is forthcoming) to purchase the *Zoological Record* as published, and to issue the separated parts to subscribers. With this view, I would ask all those desiring any part (of the volume just issued, or of past volumes) of the *Zoological Record*, to communicate with me *as soon as possible*, stating *which part* they would be willing to subscribe for.

Although it is, of course, not intended to make a profit out of this scheme, it will nevertheless be necessary to charge slightly more for the separated parts than their proportionate value, as some parts are almost certain to remain unsold.

I am confident that this scheme will not in any way injure the Zoological Society; in fact, although they maintain the contrary, I am sure it would be to their advantage to issue the parts separately, if necessary at a slightly higher rate. At present subscribers are, I believe, mostly libraries and societies, requiring the whole volume. These would, of course, continue to subscribe for all parts, even if they were obtainable separately; while, on the other hand, all specialists who do not subscribe under the present arrangements would be practically certain to purchase those parts dealing with their own subjects, if obtainable at a moderate cost. If the proposed scheme meets with support, it will go a long way towards proving the justice of the foregoing contention, and in that case it will probably be possible to induce the Zoological Society to grant the concession for which many zoologists have for long been agitating.

Royal College of Science, London.
S. Pace.

Gravitation.

In answer to Dr. Lodge's letter, I may state that Newton in his "Opticks" (Query 21) asks if an increase of density of the ether outwards from bodies will not account for gravitation, every body endeavouring to go from the denser parts of the medium towards the rarer; and if such increase of density may not, even at great distances, be effective, provided the elastic force of the medium be sufficiently great.

I do not think a tensile ether is contemplated in this theory of gravitation. Prof. Worthington's effect manifests itself only in a tensile liquid, and this constitutes its suggestiveness in connection with the hypothesis of a tensile ether. I have no such definite ideas to advance as are put forward in the "Opticks."
J. Joly.

Trinity College, Dublin.

The Feigning of Death.

The curious condition of apparent death, assumed by the English grass snake, which Mr. G. E. Hadow describes (Nature, December 6, p. 127), is one that I have frequently observed, but have always been puzzled to account for I hardly think that it has anything to do with simulation, or that it is voluntary, since I have seen snakes so affected when quite undisturbed in their cases. I have also observed precisely the same state in the common Italian snake. In my experience the condition only occurs in fairly hot weather, and when the snake has not fed for some time. This seems to point to a species of fainting fit, and I imagine that it is immediately induced by a disturbance of the cerebral circulation.
R. Harry Vincent.

Leytonstone, December 30, 1894.

Peculiarities of Psychical Research.

In reply to Prof. Pearson: (1) His remark about "scientific acumen" was not made *à propos* of M. Richet's experiments, but of those of the S.P.R.; and hardly any stress is laid on M. Richet's results, either by Mr. Gurney or Mr. Podmore. Mr. Gurney, on the contrary, expressly says: "Clearly no definite conclusion could be based on such figures." But if Prof. Pearson has made experiments which are equally striking in the opposite sense, I wish he would publish them, or communicate them to the S.P.R. (2) There was nothing in my letter to indicate that I under-estimated the importance of "abnormal distributions"; but I asked Prof. Pearson to say whether he

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had any reason to suppose such distributions might have occurred in the case in dispute. This he has failed to do—he has evaded the point. (3) Prof. Pearson descends to vague generalities except in regard to Dr. Oliver Lodge, who may be left to defend himself.

With the last paragraph of the letter, however, I heartily concur. There is nothing the S.P.R. would welcome more than intelligent and independent criticism. Only the critic would have to study the evidence first, and the Professor apparently has the "scientific acumen" to see that by doing so he would cut his own throat; for he would, *ipso facto*, become a psychical researcher! EDWARD T. DIXON.

Cambridge, December 29, 1894.

ON THE AGE OF THE EARTH.

IT has been thought advisable to publish the following documents. On October 12 I put my views before Prof. Fitzgerald and Dr. Larmor. The first paper is a copy of my letter to Dr. Larmor. It has now been edited a little, as originally it was rather hurriedly written. Some long mathematical notes, added on November 1, to prove the legitimacy of my approximate method of calculation, are now omitted, as Mr. Heaviside has given exact solutions, and has found that there is practically no difference between mine and the exact numerical answers. That Mr. Heaviside should have been able, in his letters to me during eleven days, to work out so many problems, all seemingly beyond the highest mathematical analysis, is surely a triumph for his new methods of working. Only for Prof. Fitzgerald's encouragement and sympathy, it is very probable that this document would never have been published.

I have sometimes been asked by friends interested in geology to criticise Lord Kelvin's calculation of the probable age of the earth. I have usually said that it is hopeless to expect that Lord Kelvin should have made an error in calculation. Besides, in every class in mathematical physics in the whole world since 1862 the problem has been put before students, and, as the subject is of enormous interest, if there had been any error it certainly would have been discovered before now.

I dislike very much to consider any quantitative problem set by a geologist. In nearly every case the conditions given are much too vague for the matter to be in any sense satisfactory, and a geologist does not seem to mind a few millions of years in matters relating to time. Therefore I never till about three weeks ago seriously considered the problem of the cooling of the earth except as a mere mathematical problem, as to which definite conditions were given. But the best authorities in geology and palæontology are satisfied with evidences in their sciences of a much greater age than the one hundred million years stated by Lord Kelvin; and if they are right, there must be something wrong in Lord Kelvin's conditions. On the other hand, his calculation is just now being used to discredit the direct evidence of geologists and biologists, and it is on this account that I have considered it my duty to question Lord Kelvin's conditions.

The original object of Lord Kelvin's investigation is usually forgotten. He sought to prove, and proved, that the earth is losing energy at a calculable rate. He said that the loss might be the loss of potential or chemical energy instead of sensible heat, or as well as heat, although he thought that a large proportion of potential or chemical energy was improbable; and it is only on the assumption that the earth is a cooling body losing energy originally only of the sensible-heat form, that his calculation of the age of the earth is based. Not only so, but also his earth is a homogeneous mass of rock such as we have on the surface, with the same conductivity and other heat properties. He starts with the knowledge that there is an average increase of temperature downwards in the earth of one Fahrenheit degree for every 50 feet. Assuming that the earth, a solid, was once at the uniform temperature of 7000° F., that its surface was suddenly brought to and kept at the temperature 0, and taking k/c (k being conductivity and c capacity for heat of unit volume, in year foot units) as 400, he finds that 10^8 years have sufficed to cause the temperature-gradient at the surface to be what it is now. He stated that the conditions were sufficiently represented by an infinite uniform mass of matter at 7000° F. with an infinite plane face kept at 0.

At first I preferred to consider a *globe* of 4000 miles radius of constant surface-emissivity to be cooling as if in an enclosure, kept at constant temperature. I made the emissivity infinite, and obtained Lord Kelvin's answer for temperature-gradient near the surface. When the emissivity is taken of a finite value, the time taken to produce the present temperature-gradient is less than Lord Kelvin's answer.

It is interesting to notice that if we take our enclosure to be at a zero of temperature which we can choose as we please, we have a method of using Fourier's expression in certain cases in which the emissivity is not constant. By no method of working does it seem probable that we shall greatly alter Lord Kelvin's answer.

Modification of Lord Kelvin's Conditions.

But, when we depart from homogeneity, when we assume that the interior of the earth may be of better conducting material than the surface rock in which the temperature-gradient is alone measured, we find a very different state of things from that considered by Lord Kelvin. The cooling from a constant temperature of an infinite mass bounded by a cold plane face, a slice of which near the surface is of material different from the rest of the infinite block, is a problem difficult to attack mathematically. But if the slice is thin, or if much time has elapsed, the following artifice leads to a solution.

Imagine an infinite homogeneous block, originally at temperature V_1, whose surface is kept at 0. If x_1 is sufficiently small and t great, we may neglect the exponential term, and (v being temperature and t time, and x the distance from the cold face)

$$\frac{dv}{dx} \text{ at } x_1 = V_1 \div \sqrt{\pi\kappa_1 t}; \; v_1 \text{ at } x_1 = V_1 x_1 \div \sqrt{\pi\kappa_1 t}.$$

Rate of flow of heat across unit area at $x_1 = k_1 V_1 \div \sqrt{\pi\kappa_1 t}$. I take k as conductivity, and κ as conductivity divided by capacity for heat of unit volume.

Now take another such homogeneous infinite block of different material, and use the letters with affix 2 instead of 1. Let the time be the same in both. Let the surface slice from x_1 to 0 in the first, and from x_2 to 0 in the second be considered. We can, by taking proper values of V_1 and V_2 and x_1 and x_2, make the rates of flow of heat equal and the temperatures equal at x_1 and x_2:

$$k_1 V_1 / \sqrt{\kappa_1} = k_2 V_2 / \sqrt{\kappa_2} \text{ and } V_1 x_1 / \sqrt{\kappa_1} = V_2 x_2 / \sqrt{\kappa_2}$$

Hence $k_1 \div x_1 = k_2 \div x_2$. Thus if $nk_2 = k_1$, we take $nx_2 = x_1$.

Now we can take the slice x_2 to 0 from the second block and let it take the place of the slice x_1 to 0 on the first block. The artificial block so produced will go on cooling, its outside face being kept at 0_1. But we shall have at the point of junction a sudden multiplication of dv/dx. In fact, dv/dx will be what it used to be towards the interior, but will be n times as great towards the surface. It is of no consequence what the value of κ_2 is, if times are great and slices thin, the only important thing is that k_1 shall be n times k_2. The application of the result is obvious:—

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